Comments on Klamath River Temperature, DO, Organic Matter, & Nutrient TMDL

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General Comments
Overall the document reads well, and clearly explains processes by which water quality degradation occurs. I found the explanation of *Ceratomyxa Shasta* to be very clear, and resolved some questions I had harbored about this problem in the past. Below, I limit my comments to areas in which I have background.

Staff Report, Chapter 1
p.19 Drainage density is influenced largely by infiltration capacity: highly permeable substrates will support lower drainage densities, even in areas of high precipitation. The slopes of Mt Shasta receive very high precipitation, but have low drainage density by virtue of the permeability of the volcanic rocks underlying them. Water yield is still high, but it takes groundwater pathways to springs nearby. By contrast, semiarid badlands have notoriously high drainage densities but low water yield by virtue of the dry climate and low precipitation. Thus, we would not necessarily expect the pattern of drainage density to mirror the pattern of water yield.

p.22 Text states that Fig 1.10 shows that pattern of water use has shifted timing of peak spring flows, etc – presumably this is a typo and should refer to Fig 1.11. The basis of Figure 1.11 should be better explained. How much of this figure is based on the Bureau's natural flow study? Were the mean monthly flows in Scott and Shasta Rivers integrated later or as part of the Bureau study? Note that the Bureau study did not get rave reviews from the NRC panel (NRC 2007).

Staff Report Chapter 2
General: How would the proposed revisions to the DO objectives change the frequency and duration that the river fails to meet the objectives? It is not obvious how many DO data have been collected and what patterns emerge from them. Even under pre-disturbance conditions, we would not expect the Klamath River to have the same water quality of a mountain trout stream, so a different standard is reasonable, but what exactly is the basis for the proposed standards?

p.34 *Degraded channel habitat*. Reading this section I noted that channel simplification can lead to less hyporheic exchange, but I see you brought this up later. Another consideration that should not be ignored in a conceptual model of how processes have changed on the Klamath River:
Prior to construction of the railroad in the early 20th century, during floods, the Klamath River between Klamath Falls and Keno overflowed into Lower Klamath Lake (LKL), where by virtue of its long residence time, floodwaters would have deposited suspended sediment and nutrients. Loss of this former connectivity to the lake – in effect loss of a floodplain and wetland storage function - probably produced a significant increase in flood peaks and

reduction in removal of nitrogen and other nutrients. Much of the water that overflowed into LKL probably evaporated from the shallow lake surface, but some is known to have returned back to the river when, on the recession limb of the flood, river stage dropped below the elevation of the water surface of LKL. The characteristics of this return flow were not documented, but it's likely to have been warmer than the original flood waters. The hydrologic implications of this seasonal overflow into LKL (and its loss following construction of the railroad) were not adequately analyzed in the Bureau's Natural Flow Study (NRC 2007).

p.34-35 Clarify the *effects of increased fine sediment delivery to the channel* and resultant bed fining and pool filling, versus sediment starvation and bed coarsening. On p.34, the former is cited as increasing periphyton growth, while on p.35 the latter is cited as producing the same effect (because the substrate is less mobile). Perhaps they both can produce the same result of more periphyton growth, but the mechanisms need to be explained more clearly to resolve the apparent discrepancy.

p.34 *Altered flow conditions*. Note that Copco and Iron Gate together impound only about 5% of the mean annual runoff. This is a very small *impounded runoff ratio* by California standards (Kondolf and Batalla 2005). (Compare to 80% for the Sacramento and 120% for the San Joaquin overall, higher for some specific drainages: 460% for Putah Creek, 240% for Stanislaus.) Storage by Upper Klamath Lake may be more significant, probably affecting low flows the most. It's not clear that the frequency or magnitude of scouring flows is less now than in the late 19th century, because Copco and Iron Gate would have little storage effect, and counteracting reservoir storage effects was the significant loss of flood overflow into LKL. Moreover, to have increased deposition of sediment in the river bed you need not only to reduce scouring flows, but you need a sediment source below the dam, because the dams are trapping at least the coarser fraction of the sediment load.

p.35 *Dams halt downstream transport of gravel…* The hypothesized effect is probably correct in that directly below Iron Gate substrate has significantly coarsened, as shown by surficial grain size measurements (CH2MHill 2003). It is possible to scour periphyton from stable cobble beds by transporting sand over them, but sand is trapped by Iron Gate Reservoir so the reach immediately below the dam would be starved of sand. Note that this effect would persist downstream only until tributary contributions of sediment became significant. Below Iron Gate, Bogus Creek delivers enough gravel to the mainstem (some of which is exotic gravel placed in the channel to improve spawning habitat in the tributary) to produce mobile gravel bars starting just below the US Geological Survey gauge, about 100m downstream of the tributary confluence.

p.36-37 *Thermal processes related to sediment load.* It seems the document is arguing that several separate processes occur. It might be useful to clearly distinguish them, as the reader is likely to conflate them now.

The first paragraph refers to "…pool filling, increased width, decreased depth, and/or reduction of intergravel flow."

The second paragraph notes that sediment can fill pools and narrow channels, so that the river flows over an aggraded surface in what will be a wider channel. Simply by virtue of the increased width (and thus reduced average depth) we can expect more exposure to solar radiation and greater heating.

The second paragraph notes that aggradation can result in loss of riparian vegetation, but the mechanism is not stated. Is it because the aggraded channel exerts more erosive force on banks and undercuts them, causing riparian trees to fall into the channel? (In this case we should probably give some credit to the increased complexity that might result from the large wood in the channel.) Is it because the aggraded channel raises the water table in the adjacent banks and waterlogs riparian trees adapted to better-drained conditions in summer months? Whatever the mechanim(s), explain this better, and if this point is drawn from Lisle's work, cite accordingly.

The third paragraph expands on why a wider, shallower channel will gain more heat in the daytime (and lose more at night). The Poole and Berman (2001) citation is incomplete in the References Cited as only the authors and title are included in the citation, not the journal or report series. Presumably this report documents some of Poole's work in eastern Oregon, where bed complexity is a primary driver of hyporheic flow and moderation of diurnal temperature fluctuations (Poole et al. 2006). This is another mechanism, and should be clearly distinguished from the channel becoming wider and shallower, as it pertains to the form of the longitudinal profile, rather than the cross section.

Channel simplification that reduces the undulations in the bed, can reduce the exchange of surface and groundwater. Two recent studies have documented that more complex channels with significant bed undulations (e.g., pool-riffle alternations) have more hyporheic exchange and moderated diurnal temperature fluctuations. Alicia Arragoni's masters thesis research on the Umatilla (with Poole) documents the moderating effects on diurnal temperature fluctuations of complex bed topography. I believe her research has appeared in Water Resources Research by now, though I have only a a draft version on my computer (Aragoni et al, submitted), which I attach. Mark Tompkins' PhD research (2007) documented hyporheic exchange in complex reaches reduced diurnal fluctuations by 2oC or more on Deer Creek in Tehama County.

The second paragraph on p.37 alludes to reduced permeability, which would result from deposition and infiltration into the bed of finer sediments (silts, clays), but this point is not developed. There are examples in the literature of side channels whose groundwater exchange has been blocked by a surficial layer of silt, such as along the Rhone River in France, where an overlying silt layer was removed explicitly to restore hyporheic exchange (Henry et al. 2002). This has probably occurred in some places in California and Oregon, but I cannot think of an example now. If there is any evidence for such effects on the Klamath or its side channels, this would be useful to present in the TMDL. Also in Australia, 'sand slugs' have reduced hyporehic exchange in many streams (Boulton et al. 2002).

The third paragraph on p.37 argues that "…streams with prominent pool-riffle morphology exchange more heat via conduction than flat, simplified stream channels." Perhaps this could be more clearly stated in terms of greater exchange of surface water with the reservoir of shallow groundwater in the alluvial underneath the stream channel, whose temperature is not subject to wide diurnal fluctuations. Expressed in terms of conduction, the argument is unclear and might confuse readers.

p. 37 *Thermal processes related to flow* It may be worth noting that this simple model of more water flowing faster down the channel lies at the heart of most temperature models, but does not account for channel complexity and resulting thermal refugia. In some cases, thermal refuges like ‘cool pools’ function better at lower flows because they remain more hydrologically isolated from the warming main-channel flow. At higher flows, the feature isolating the cool pool (such as a log or topography) can be drowned our, leading to mixing with the warmer mainstem waters. The statement, “These principles are true for any stream” might imply that the simple model explains all, which could mislead some readers. On the San Joaquin River, temperature models based on the concept of a volume of water moving downstream through a hot valley indicated that to keep temperatures below lethal levels would require volumes of water that simply did not exist in dry years – but we know from historical data that fish successfully migrated in those years. Clearly the fish were able to adapt by seeking out cooler parts of the channel, moving at night, etc.

p.45 *Temperature* It is known that salmonids near the southern end of their range in warmer waters of California have adapted to higher temperatures, and may actually do better at temperatures above the ideal ranges identified from studies in the Pacific Northwest and Alaska, where fish are adapted to cooler waters (see Williams 2006). Perhaps the text could address this point to clarify whether these geographical adaptations were accounted for in literature used to develop these temperature thresholds.

p.70, *second paragraph, streambed armoring*. Armoring of the streambed on the Klamath River is the result of trapping of sediment by the upstream dams, not alteration of the flow regime by dams. As noted earlier, Copco and Iron Gate together impound only around 5% of the mean annual runoff and have not reduced peak flows very much, but they do effectively trap all bedload sediment. Moreover, other things being equal, one would expect the greatest armoring below dams that do *not* reduce high flows (like Copco and Iron Gate) because these reaches still have the energy to transport sediment but have lost their coarse sediment load to upstream reservoirs (Kondolf 1997). Dam-altered flows could affect spawning habitat by changing the depths and velocities over the spawning gravels during spawning season, but for the Klamath dams the available data do not support the hypothesis that the flows themselves have altered availability of gravels (CH2MHill 2003). If there is any alteration to the Klamath flow regime that would have increased scour of the gravels below the dams it would be the elimination of overflow into Lower Klamath Lake, which as discussed above, must have increased peak flow magnitude in the channel.

p.70, *third paragraph, tributary deltas*. Formation of deltas at tributary confluences is probably attributable to pulses of sediment from the tributaries, rather than reduced competence and transport capacity of the mainstem due to dam-induced hydrograph alterations. (The dams haven’t changed the high flow regime appreciably.) It’s possible to get such tributary delta formation simply from increased sediment yield from tributaries, even if there is no change in the mainstem high flow regime.

p. 70, *debate between second and third paragraphs*. Note that these two paragraphs imply contradictory conceptual models, though they are not spelled out. Paragraph 2 implies that transport competence and capacity have been increased by the dams (more scour of gravels) while Paragraph 3 implies that they have been reduced (less ability to mobilize sediments delivered from tributaries).

Staff Report, Chapter 3

p. 13-14, *Scott River flow and temperature*. I found the discussion of interactions among surface flow, groundwater, and water extractions in the Scott Valley to be informative, not knowing much about this topic in advance. I may have missed something in my reading, but it is not clear to me what data constrain the model assumptions here. What temperature data exist, for what locations, etc? Perhaps the document would be more credible if specifics regarding available data and interpolations/estimates needed were spelled out in lieu of terms such as "moderate amount" such as in the passage, "These estimates are based on a moderate amount of verifiable information, couple with reasonable assumptions about the hydrology of the Scott Valley." The next sentence refers "uncertainty"; to what extent can it be quantified?

p.15 *Trinity River temperature.* I'm surprised there are not better temperature data for the Trinity, given the degree to which it's been studied. Again, perhaps a clearer statement of what is constrained by data, what kinds of interpolations/estimates were required, and what uncertainties would result, could improve the document.

Staff Report, Chapter 4

Figures 4.1-4.3 seem very effective ways to communicate the conceptual model of nutrients inputs. Can the figures (or supporting text) be modified to indicate which numbers are based on actual field measurement programs and which values are interpolated/estimated? Some indication of the uncertainty in these values?

p. 33 *thermal refuges at cold-water tributary mouths*. The effect of increased tributary sediment loads filling in cold water refugia appears to be an important effect. Any citation to support the last sentence of paragraph 2?

Appendix 4 Fisheries

This section appears to be a good summary of available data on status of fish in the basin overall. Figures 2-4 are interesting but somewhat difficult to read. Perhaps they would be more readable if the lines showing reaches where fish persist were to be different shades or thicknesses of blue or green, while reaches where fish were extirpated were shades of red or orange.

Appendix 5-D Determination of Tributary Flow

The approach presented is reasonable as a first cut, but the explanation seems to leave many questions hanging. First, the net increase in flow from one gauge to the next is attributed to the intervening tributaries, and the water yield is assumed to be a constant per unit area, i.e. tributary responsible for 40% of the increased drainage area is assumed to produce 40% of the increased flow. Lacking any information beyond drainage area, this is reasonable, but precipitation is highly variable spatially, so it would seem that an isohyetal map should be consulted to assess the degree to which this simplification might result in significant over- or under-estimates in allocation of flow to individual tributaries. Second, the USGS method involves monthly averages, whereas the TMDL model used 7-day average values. How exactly was this done? For each water year, were days 1-7, 8-14, 15-21, etc averaged? (i.e., Oct 1-7, Oct 8-15, etc) How different were the results for high-flow months vs baseflow months? (I would expect some significant differences.) And finally, who is the mysterious “Mr. M, Flug”?

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